1873MNRAS..33R.536P

It is *absolutely impossible in my opinion to distinguish one from the other, or to ascertain in what degree one or other operates, except by the proposed method of star-gauging, which combines, be it noticed, the qualities of the two methods suggested by Sir W. Herschel* (*one in* 1784, *the other in* 1817), *without being liable to the causes of error which exist in either of those methods. The general principles of interpretation, applicable not only to star-gaugings with different instruments, but to all differential observations, whether relating to colour, to duplicity triplicity or grouping generally, to spectrum, to motions whether thwart or in the line of sight, to peculiarities of configuration, to the various orders of nebulæ, or so on, are simply these,*—

(i.) *Where two orders of observation indicate concordant laws of distribution over the heavens, the rich regions so indicated are regions of* SPACE *where the two orders of objects are intermingled so as to form parts of one system.*

(ii.) *Where the results are in direct contrast* (*or discordant in the true sense of the word**), *the rich regions for one order corresponding to the poor regions for the other, and* vice versâ, *the two orders of objects belong to one system, but some peculiarity in their nature, or in the laws according to which they were formed, causes them to occupy different parts of the system, segregating as it were from each other.*

(iii.) *Where no connexion whatever, either of agreement or contrast can be observed, it is probable, and in general presumable, that the two orders are altogether distinct and lie at different distances from each other.*

And, lastly (iv.), *whether partial or local agreement or contrast is indicated, then the inference is that the true arrangement of the objects in space is affected both by laws of aggregation or segregation and by diversities of distance, and by one cause or the other to a degree indicated by the extent of such agreement or contrast.*

Note on the Expulsion Theory of the Solar Corona, Comets, &c., with special reference to Prof. Norton's original enunciation of that Theory. By Richard A. Proctor, B.A. (Cambridge), Honorary Fellow of King's College, London.

I have for some time intended to present to the Fellows of this Society a *résumé* and discussion of the papers of Prof. Nor-

---

* Which, however, is ordinarily used in too vague a way to be suitable as the converse of concordant.

1873MNRAS..33R.536P

ton, of New Haven, Conn., U.S., on the subject of the theories respecting the corona, comets, &c., recently advocated by myself, —partly with the intention of indicating his claim to priority, and partly because of the intrinsic interest and importance of his remarks. But a great and increasing pressure of engagements has prevented me from doing so in a permanent form, though I have on several occasions indicated the state of the case in lectures.

I must now leave to another opportunity and another place the preparation of a full account of Prof. Norton's views; but it appears to me desirable to indicate his claim to priority, because it was in these pages that I first advocated the theory (which, however, I have not described as mine, having, in fact, distinctly stated that it was *adopted and advocated*, *not conceived*, by me.)

In a letter to me, Prof. Norton writes as follows: "The principal feature of the auroral theory which I have advocated is that the corona is made up of matter in the act of streaming off from the Sun, instead of being a permanent solar atmosphere, or a mass of revolving meteors, and this is precisely the great distinguishing feature of your theory that the corona is a phenomenon of eruption. The theory of an outstreaming of solar matter I propounded in the second edition of my *Astronomy*, published in 1845. I there say, 'The explosive actions which are the most probable causes of these spots, may perhaps furnish the luminary matter, which may afterwards be driven off to an indefinite distance by some repulsive action of the Sun. Certainly if there is at the Sun's surface any matter of the same nature as that of which the tails of comets are composed, it must be expelled by the same repulsive force that drives off this species of matter from the heads of comets and forms their tails.' The auroral theory of the corona is distinctly set forth in the edition published in 1867. The outstreaming of solar matter is a prominent feature of it, as appears from the following paragraph. 'The photospheric matter, dispersed, by reason of the varying action of the planets, sufficiently to become subject to a repulsive action from the Sun, as it flows away into space, forms the *corona*, with its accompanying radiations and streamers, visible in total eclipses.' I also (p. 178) take the ground that the zodiacal light 'is made of the streams of particles flowing away from the Sun, under the operation of a force of solar repulsion; or, in other words, is the indefinite continuation of the corona observed in total eclipses of the Sun, with its attendant streamers.'

"Without specifying farther what I have urged in support of this doctrine of visible material emanating from the Sun, I will refer you to the statements in the article on the corona which I send you by the same mail with this letter. I also send my paper 'On the Periodical Variations of the Declination and Directive Force of the Magnetic Needle,' published in the American *Journal of Science* (March and July, 1855), in which you will see (p. 47, &c.,) that I undertook to explain the irregu-

1873MNRAS..33..536P

lar perturbations of the magnetic needle by electric currents developed in the upper atmosphere (or 'photosphere') of the Earth by the arrival of the solar matter, and took the ground that 'the matter radiated from the Sun, and falling upon the Earth's atmosphere, was probably the substance of terrestrial auroras.'

"I send you, also, the principal portion of my paper on Donati's comet, published in the *Journal of Science* (July, 1861), by which you will perceive, if I mistake not, that my investigations with reference to comets are at least worthy of recognition. In the second edition of my *Astronomy* (1845) I advocated, at considerable length, the theory of the development of the tail of a comet by the operation of a repulsive force exerted by the Sun, giving the credit of the origination of this theory to Olbers. On looking over the article on Donati's comet you will see that I have subjected the theory to the rigorous test of comparison with observation, and that I have shown, by careful computations, first, That the great cause of the wide lateral dispersion of the matter of the tail was an inequality in the Sun's force of action on different cometary particles; secondly, that I have assigned the actual limits between which the force varied; thirdly, that I have made out that not far from one-half the tail in breadth, on the concave side, was made up of particles that *were not effectively repelled by the Sun*, but separated from the nucleus, after having become disengaged from its influence, only because they gravitated toward the Sun with less force than the nucleus did,—a result certainly of great importance, and which, so far as I know, has not been to this day reached by any other investigator; fourthly, that all the particles variously acted on by the Sun, which left the head of the comet simultaneously, were, after the lapse of one, two, three, and five days, for a continued interval of some three weeks, distributed along successive *right lines diverging from the head* (see fig. on p. 61.) This result furnishes an explanation of the 'columnar structure of the tail of the comet' noticed by Bond and other observers, showing that it must have resulted from variations, from day to day, in the quantity of cometary matter ejected from the head. It also gives us an insight into the phenomenon of 'secondary tails;' they are to be regarded merely as lines of matter *variously* repelled, but, for the most part, by a much more energetic force than the maximum repulsion that prevailed along the preceding side of the principal tail. The results of the observations do not, however, require us to suppose that the particles at the extremity of the secondary tail (which were those most energetically repelled) had a higher average velocity than about one hundred miles per second; for the observed positions of the secondary tails show that they were made up of matter which left the head not less than three or four days before the date of observation (Oct. 5).

"I ought here to take occasion to explain an apparent error

1873MNRAS..33R.536P

in my calculations. On page 65 it is stated that the actual position of the nucleus was 18′·6 in advance of the calculated position. Soon after the publication of my article, I received from Prof. Bond a letter bearing date July 10, 1861, of which the following is an extract: 'I have just received the *American Journal* for July, and, in looking over your interesting article on the comet of Donati, see at once that in the chart on p. 64 you have taken 7^h Gr. for the time to which it corresponds, instead of $8^h\ 30^m$ Gr., for which it was actually projected.'

"The error was, I have no doubt, an inadvertence on my own part in the copy sent you. The correction will be easily made, as the tail moved, through nearly its entire length, on that day with a daily motion of $4\frac{1}{10}°$ normal to its curve. The nucleus itself 4°. Making the correction referred to the calculated and actual positions of the nucleus differ only $3\frac{1}{2}'$."

Statement of Views respecting the Sidereal Universe. By Richard A. Proctor, B.A. (Cambridge), Honorary Fellow of King's College, London.

It appears to me desirable to conclude my discussion of the relations presented by the Sidereal Universe—so far, at least, as these pages are concerned—by indicating briefly but distinctly the points in which my views differ from the views advanced by those who have preceded me in the discussion of this subject.

I must premise that Sir John Herschel, the late Earl of Rosse, Whewell, and Herbert Spencer have advocated more or less decidedly some of the opinions which I have been led independently to enunciate. Regarded as a whole, however, my views are original, and several of the points to which I draw attention were first noted, I believe, by myself. If not, I would desire to be corrected, so as not to present as new views opinions which have already been advocated by others.

In the first place, then, I lay down as a fundamental rule that no *hypothesis* as to star-magnitudes or star-distribution can safely be adopted as a basis of research. In ordinary subjects of inquiry, it is well to have working hypotheses, varying perhaps as we proceed, but serving conveniently for the co-ordination of observations. But in researches into the constitution of the Stellar Universe, we must not adopt any hypothesis until observations sufficiently numerous and extensive have indicated its justice.*

---

* It is rather singular that my inquiries, *the first systematic inquiries ever based on observations as they stand*, *without assumption of any sort*, should have been so misunderstood by several as to be described as theoretical by comparison with such inquiries as the earlier work of Sir W. Herschel, based on assumptions